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December 14, 1854.

The LORD WROTTESLEY, President, in the Chair.

Robert Mallet, Esq., was admitted into the Society.

The following communications were read:—

I. "The Physical Theory of Muscular Contraction." By CHARLES BLAND RADCLIFFE, M.D., Licentiate of the Royal College of Physicians, Assistant-Physician to the Westminster Hospital, and Lecturer on Materia Medica. Communicated by CHARLES BROOKE, Esq., F.R.S. Received November 18, 1854.

The theory set forth in this paper is, that muscle is *prevented from contracting* by the several vital and physical agencies which act as stimuli upon muscle,—volition, nervous influence, blood, electricity, light, heat, and the rest,—and that *contraction happens on the cessation of stimulation*, by virtue of the operation of that universal principle of attraction which belongs to muscle in common with all matter, and, so happening, that it is a *physical* phenomenon of the same nature as that contraction which takes place in a bar of metal on the abstraction of heat.

This theory is supported by various arguments, some of which are now stated for the first time. It is argued:—

(*a.*) That *nervous influence* cannot cause muscular contraction, (1) because the degree of innervation, as measured by the supply of nerves, is inversely related to the tendency to contraction; (2) because contraction does not take place so long as the nerve gives evidences of electricity (Du Bois Reymond); (3) because, in some instances at least, contraction does not happen so long as the nerve gives evidences of "irritability"—for contraction is not caused by

heat, by acids and alkalies, and by several other chemical and medicinal substances, until the possibility of provoking contraction by the touch of a needle has been destroyed by the action of the agent—until, that is to say, the "irritability" of the nerve has been destroyed by this action (Eckardt); and (4) because the influence of the nervous centre in causing contraction is to suspend the natural electricity of the nerve and muscle. This last conclusion is evident in the fact, that the signs of electricity, which are absent during tetanus, immediately reappear in the muscle and in the portion of nerve connected with it, when the influence of the nervous centre is cut off and the tetanus resolved by dividing the nerve.

(*b.*) That *blood* cannot cause contraction, (1) because the tendency to contraction is inversely related to the supply of blood; thus, this tendency is greater in the voluntary muscles of fishes and reptiles than of mammals and birds—greater in involuntary than in voluntary muscles—greater in the muscles of any given animal during the state of hybernation than during the period of summer life; and (2) because the state of *rigor mortis* may be relaxed more than once, and the lost "irritability" restored to the muscle by the injection of living blood into the vessels (Brown-Séguard).

(*c.*) That *electricity* cannot cause contraction, (1) because there is a constant current of electricity in a muscle during rest, *but not during contraction* (Du Bois Reymond),—because, that is to say, contraction is absent when muscle is in a state of electrical or *polar action*, and present when this state is absent, so that contraction appears to be antagonized by this state of polar action; and (2) because contraction is never coincident with the passage of a current of artificial electricity; for, not only is it true that a muscle does not contract during the time that a current of artificial electricity is passing through it, but *contraction is invariably relaxed if contraction pre-existed* (Eckardt). There is, indeed, momentary contraction at the opening or at the closing of the circuit, but this contraction can be shown to be coincident with neutralization of electrical action, which neutralization is consequent upon the momentary opposition of the natural current of the muscle and the artificial current.

(*d.*) That *mechanical agents* cannot *stimulate* contraction, (1) because the electrical phenomena of muscle are opposed to such an idea; thus muscle affords evidences of electricity during rest, but

not during contraction, and hence the probability is that electricity has been discharged when a muscle contracts on being touched by a needle,—a probability which is supported by the analogy which exists between the structure of muscle and the structure of the electrical organ of the Torpedo, and between the circumstances producing contraction on the one hand and discharge on the other (Owen, Faraday, and others); and (2) because the movements of the stomach, or uterus, or any other viscus are not to be accounted for on the supposition that the contractions are stimulated by the contents of the viscus; thus the food accumulates and the stomach expands until the appetite is satisfied, and contraction does not happen until the preliminary processes of digestion are at an end, and thus also the child grows and the uterus expands, and labour pains do not begin until the growth of the child is completed, and the *stimulus* of that growth suspended.

(*e.*) That *heat and cold* do not *stimulate* contraction, because contraction does not happen until the *natural polar action* of the muscle is suspended,—an event which happens equally under either extreme of temperature,—and thus the muscle would seem to contract because the heat or cold extinguishes that polar action of the muscle which antagonizes contraction.

(*f.*) That *light* cannot cause contraction, (1) because it exercises a directly opposite influence upon the irritable cushions of the sensitive plant; and (2) because it is as easy to agree with Bichât, and suppose that light expands the curtain of the iris, as that it causes contraction in sphincter-fibres surrounding the pupil, which fibres have no existence.

(*g.*) That *chemical and mechanical agencies* do not stimulate contraction, because contraction does not happen until the agent has destroyed that polar action of the muscle which antagonizes contraction (Eckardt).

It is argued, also, that the action of the *will* upon muscle is not necessarily that of a *stimulus*, for the will *may act* by withdrawing something from the muscle as well as by communicating something to the muscle, and, if so, then the previous considerations enhance the probability that it acts by withdrawing something.

In the course of the argument it is further shown that this con-

clusion is borne out by the history of the muscular movements which are manifested in the coats of vessels and in the heart, while at the same time this view is found to give the clue to the physical interpretation of "capillary action," and of rhythm, whether this be in the heart or elsewhere.

It is shown, also, that the same conclusion is borne out by the pathology of tremor, convulsion, and spasm,—of those diseases, that is to say, in which muscular contraction is in excess. Thus, (to mention one argument out of many,) the state of circulation which is invariably associated with tremor, convulsion, and spasm, is one which necessarily implies the diminution of all accustomed stimulation in the muscle, for it is a state which borders closely upon syncope or asphyxia.

And, lastly, it is shown that there is nothing in the phenomenon of muscular contraction which need prevent it from being referred to the operation of that common principle of attraction which belongs to muscle *in common with all matter*, and thus the general conclusion is that another barrier between the organic and inorganic world is broken down, and that muscular contraction is an effect of the universal law of gravitation.

There are, however, sundry grave objections to this theory, and one main object of the paper under consideration is to remove them. Thus, for example, if muscle contracts when nervous influence is withdrawn, how is it that it relaxes when the nerve is divided or otherwise paralysed? and if a muscle contracts for want of blood, how is it that it relaxes in syncope, asphyxia, and death? These objections are grave, but not unsurmountable, as the following hints at explanation will serve to show.

It must be understood, then, that that state of polar action which is present in a muscle during rest and absent during contraction, *is re-established immediately after contraction*; it must also be understood that this state of polar action in the muscle is suspended during ordinary muscular contraction by certain changes which take place in the nervous centre, and that it has *died out* when contraction happens after death, as in *rigor mortis*; and the rest is sufficiently simple.

It is quite in accordance with the theory, then, that a muscle should contract when nervous influence is withdrawn, and that it

should relax after the nerve is divided or otherwise paralysed. At the moment when the continuity of the nerve is broken the muscle contracts, because the influence of the nervous centre is cut off; but this contraction cannot continue, because that state of polar action which antagonizes contraction is immediately re-established in the muscle, and in the portion of nerve connected with it. This relaxation, moreover, must continue, if the paralysed muscle be left to itself, so long as the muscle continues to be the seat of this polar action. And, on the other hand, this contraction must return when this action is suspended, or diminished, or extinguished, as indeed it does; thus the muscle contracts when the polar action is suddenly suspended by galvanism or by the touch of a needle; thus it contracts after the paralysis has continued for some time, and when the failure in the nutrition of the muscle has entailed a corresponding failure in its polar action; and thus it contracts in *rigor mortis*, when all polar action is finally extinguished.

It is also in accordance with theory that tremor, convulsion, and spasm should be caused by want of blood, and that they should cease when the circulation fails, as it fails in syncope, asphyxia, or death. During tremor, convulsion, or spasm, the muscles are insufficiently supplied with nervous influence, because the deficient supply of blood to the nervous centres involves a corresponding deficiency in the degree of innervation; but once let the circulation fail below a certain point, and the whole case is altered. During tremor, convulsion, and spasm, the supply of blood to the nervous centres is insufficient to keep up the normal degree of innervation, *but it is sufficient to prevent the nerves from being paralysed,* and hence the contractions in the muscles, for the nerves being conductors, the failure in the action of the nervous centres is propagated along them to the muscles, and of this failure the contractions are the consequence. But if the circulation fails below a certain point, *the nerves are paralysed for want of blood,* and being paralysed, the failure of innervation in the nervous centres, even though this be now complete, does not entail a corresponding failure in the polar action of the muscle, because the nerves are no longer conductors; and not doing this, the polar action of the muscle, which is much more vigorous than that of the nervous centre and nerve, and far less dependent upon the supply of blood, is immediately re-established,

and being re-established, the muscle relaxes (just as it does in the case where paralysis is caused by division of the nerve), and tremor, convulsion and spasm are at an end. Nor is there any doubt that the nerves are paralysed when the circulation fails to the point which is here supposed. Thus, if the circulation in the hand be depressed by immersion in cold water, the sense of touch and the power of movement are partially or wholly destroyed; or if the principal vessel of a limb be tied, the nerves are similarly paralysed until the collateral circulation be established; and in each case, also, the power of provoking "reflex movements" is diminished or destroyed. In either case the nerves are more or less paralysed for want of blood, and, if so, it surely follows that the nerves must be paralysed, and still more effectually, when the circulation fails as it fails in syncope, asphyxia, or death, and when the movement of the blood is almost or altogether at an end. Hence it is quite intelligible that tremor, convulsion or spasm should be caused by want of blood, as is stated in the argument, and that they should cease in syncope, asphyxia, and death; and thus this objection falls to the ground, and with it all objections of the same kind.

Such is an imperfect sketch of the evidence upon which the physical theory of muscular contraction is founded.

II. "On the Structure of some Limestone Nodules enclosed in Seams of Bituminous Coal, with a Description of some Trigonocarpons contained in them." By J. D. Hooker, M.D., F.R.S., and E. Binney, Esq. Received November 23, 1854.

The authors first describe the occurrence of the limestone nodules, which form a continuous bed in the centre of a thin seam of bituminous coal in the lower part of the Lancashire coal-field. The nodules were of various sizes, some weighing many pounds, and caused the coal to bulge out both above and below them, and they were found to be entirely composed of vegetable tissues converted into carbonate of lime and magnesia. Their formation is supposed by the authors to be due to infiltration of water through the superin-